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Commentary

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The contribution of Färe, Grosskopf and Margaritis employs a productivity indicator that has not been much used so far; what is termed a Bennet-Bowley productivity index. It is stated that this index may be derived from a Luenberger productivity indicator based on directional technology distance functions. My major concerns are:

(i) A so-called Bennet index is used, using price data that is not discussed or shown. The index is not explained and compared with alternatives, e.g. the Törnqvist index.
(ii) How R&D expenditure translates into productivity is not discussed, e.g. the role of lags and the role of human capital.
(iii) Why is only private R&D used? One would think that public research is important (agricultural universities are run by the state even in the U.S.).

My first concern relates to the historical view of the Bennet-Bowley productivity index. There is a reference to Bennet (1920), but not to Bowley. One must be an insider to understand how Bowley got involved. Reading Bennet it may be that Bowley (1919): "The measurement of changes in the cost of living," *Journal of the Royal Statistical Society* 82(3), 343–372, is relevant, but Bennet only uses his data. It is not easy to see what Bennet proposes, anyway.

It is stated that directional distance functions are appropriate measures of total factor productivity in a profit maximising setting because directional distance functions credit expansion of outputs and contraction of inputs. This argument seems to be beside the point. What is involved here is the choice of a frontier technology as a reference for technology and then productivity measures are based on some relative measures to the frontier from the observations. As becomes evident later profit maximising conditions are not involved in the construction of the Luenberger index. This is understandable, for in order to do this it must be explained why units do not realise the frontier technology when maximising profits. A competing model is the Malmquist index. It is not explained why directional distance functions are preferred to radial distance measures.

There is no motivation as to why we should be interested in the profit function in the setting of agricultural data assuming existence of inefficiency. It seems that the unit is projected to the frontier when the profit function is defined. If so, this kind of profit function is purely hypothetical.

There are several points that require some elaboration and clarification. The authors refer to a productivity shock. But using yearly data, what is the definition of a shock? This is not addressed in the paper. There is no discussion on how to choose the directions g. The motivation for the choice, or potential choices, is needed. In footnote 7 the function in (16) is called an index. It is difficult to see why. The Malmquist index is related to the Luenberger index, but it is not motivated why this should be of interest in this paper and its analysis. Finally, the use of Granger causality test is very mechanical. What one would expect is a discussion about the relationship between R&D and productivity. Can the authors elaborate on this?

The second contribution by Onofri and Fulginiti sets out to investigate the impact of public R&D and public

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infrastructure and the performance of U.S. agriculture using two different time series covering 1948–1994 and 1926–1990. My comments focus on the following issues:

Why do policy makers need shadow prices for public capital since they adjust optimally by assumption?

Regarding the steady state, how long time does it take to reach this level? How does one know that steady state is reached before the public sector changes public capital again?

It is not mentioned that the problem in (1) assumes that the path of output has to be given. How can the firm know the output path to infinity? The assumptions clarifying the character of the dynamic cost minimization should be articulated further.

The role of government assumed here is very far from any real life situation. Assuming that public capital does not affect consumers is very special: what about telecommunications, roads, schools, hospitals, etc? Welfare optimisation is mentioned several times, but what good is this without consumers? In any event, how can the government know about the value functions of the private sector to infinity? I have difficulties understanding that the public sector in order to know the private value function must figure out the output path of the private sector including the private sector's reaction to the change in public capital. Is this really what is modelled?

The public cost of adjustment is now the reallocation of an investment fund, not the accumulation of public capital. Why should there be waste in the reallocation of public resources? A public sector that can calculate correctly the private sector up to infinity should hardly be expected to waste resources.

Footnote 4 suggests that the total amount of public capital is used, not only what is relevant for agriculture. But how much of public investment is motivated by agricultural needs? To capture effects may be the aggregate private economy must be specified. If public R&D spending also is the total I wonder if the data really is suitable.

There are some final points requiring elaboration. Footnote 21 refers to farmers not anticipating that prices are going to change. How come the farmers do not get the idea that prices are going to change and build that into their expectations mechanism? In reference to R&D stocks, R&D is measured as yearly expenses. What do stocks mean in this context? There is no information about inventions or innovations or human capital or something that may be stocks. Badly behaved technologies are mentioned. How should the reader react to this information? Private R&D and extension expenditures: what does it mean that inclusion of the data "strain the structural estimation even more." What conclusions should we draw of the information given that a simultaneous estimation of public and private investment is not done?

The declared novelty of the final contribution by Paris is that relative prices enter the production function as shifters of the technology frontier. After introducing this idea and working out the consequences for Shephard's lemma etc., the model is applied to the agricultural sector as an aggregate for an 80-year period. The introduction of relative prices is not a novelty, but has been done by the author in two other published papers already (Paris and Caputo 2001; Caputo and Paris 2005).

The Hicks conjecture of the impact of relative prices on technical change is an interesting one. But I am not sure the author is on the right track. First of all, the author's concept of relative prices is an unusual interpretation. The common understanding is that relative prices mean price ratios of inputs. Deflating input prices by the output price seems beside the point. More seriously, what is leading to changes in relative prices? One way it may work is that the nature of inventions is such that demand for inputs is affected in different ways. Then it is not relative prices that influence technical change, but the *opposite*. The idea of the relative prices as technology shifters breaks down. According to this story the only way forward is to collect data on the inventions themselves, and to study their nature as to input saving, factor augmenting, output increasing, etc.

Accepting the idea of prices as technology shifters I am not sure about the way the author interprets this when reworking Shephard's lemma. My problem is that all the variables are dated to the same time and that the firm solves a static optimisation problem. But then only *one* technology can exist that is relevant for optimisation, and it is impossible to separate classical substitution effects from the technology shifting function as is the story told by the author. How can you operate with technical progress in a static environment? To me it seems that the author confuses the ordinary substitution effect that is change in *technique*, and technical change that is *shift* in the production function. But keeping prices in the production function you surely complicates Shephard's lemma with all the mathematical complexities shown by the author for the third time in a publication.

The econometric model seems to grow out of the control of the author, dividing the estimation into two phases leaving me with uncertainty as to how to respond to the problems of the first phase. The number of constraints and parameters seems to grow out of all manageable proportions. The way the testing for the effect of prices is done in the second phase makes me uneasy: it is economic theory that should dictate the model, not econometrics where adding more or less strange variables may increase the value of the likelihood due to spurious correlation.

It is interesting to note that in Caputo and Paris (2005) it is argued that the model should be applied to firm data. How appropriate then is it to apply this model to aggregate data?

References

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